THE ANNALS

AND

MAGAZINE OF NATURAL HISTORY.

No. 127. MAY 1847.

XXXII.—*Further Observations on the Formation of the Flints of the Upper Chalk, with Remarks on the Sponge Theory of* Mr. Bowerbank. By J. Toulmin Smith, Esq.

It is well remarked by Bacon*, that, "if every intellect of every age could assemble and labour in united and transmitted union, but little progress could ever be made in science by the method of *anticipation.*" And in another place† he tells us what he means by this "anticipation" (which term he uses as the opposite to "interpretation‡," or true generalization), when he says, "There are two ways of seeking truth. One jumps from a few individual facts to general axioms, and makes use of such axioms in all other individual and mediate cases; and this is the way hitherto in use. The other draws axioms from facts also; but it is by going gradually up from one to the other, by slow steps, until at length the general axioms are reached; and this is the true though untrodden way."

Sir J. W. Herschel says§,—"Whenever we perceive that two or more phænomena agree in so many or so remarkable points as to lead us to regard them as forming a class or group, *if* [there is much in this *if*]—if we lay out of consideration, or abstract, *all the circumstances in which they disagree,* and retain in our minds *those only* in which they agree; and then, under this kind of mental convention, frame a definition or statement of one of them in such words that it shall apply equally to them all, such statement will appear in the form of a general proposition, having, in so far at least, the character of a *law of nature.*" A law of nature may perhaps be defined as being a proposition announcing that a certain class of individuals agreeing in *one* character agree also in *another.*

What is necessarily implied in the very term "law of nature," and in the language of Bacon and Herschel? Obviously that there exists no single thing or fact in nature which is simply and independently existent in itself; which does not stand in the relation of *necessary connexion* with some other thing or fact in nature. The recognition of this as a primary axiom certainly lies at the bottom of all science and of every attempt to discover truth. If truth or science has any existence, and is not a mere vain search, every question must really assume this form :—Given the thing or fact before me, with what other thing or fact does it stand in necessary connexion or relation? Abstractedly few men will deny this; but practically it is neglected every day. It is now as it was in the time of Bacon, that the reasoners by "anticipation," they who "jump from a few individual facts to general axioms, and make use of such axioms in all other individual and mediate cases," are most common; the important *if* of Sir J. Herschel is forgotten. Were not this the case, we should never hear, as we continually do, of exceptions to laws of nature. Every one would feel, at the very beginning of and throughout his investigations and attempted generalizations, that in facts of nature there is a necessary connexion, or there is not. If there is, no exception can exist; for the statement of such exception involves a contradiction, viz. that there is *not* a necessary connexion.

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* Nov. Org. Scien. lib. i. § 30 (ed. Lugd. Bat. 1645).

† *Ibid.* § 19. ‡ *Ibid.* § 28. § Preliminary Discourse, p. 98.

Every process of true induction must depend upon the exercise of three different mental operations :—comparison of similitudes (or analogies); of dissimilitudes, or points of disagreement; and, lastly, inferring a necessary connexion between the existence of and relations common to the things or facts compared. Most men are caught by analogies; on these frame theories—"*anticipations*;" with a distorted axiom thus obtained they view every fact subsequently seen, caught, in each, by its similitude, disregarding its dissimilitudes, and so never arrive at a true "*interpretation*." Talk to such men of the very first principles of reasoning, of the primary axiom of "necessary connexion," or, as the same thing may be stated in other words, of the universal laws of unity and design, and they grow impatient. No wonder at it: the constant application of those principles must upset many a theory. Every one who has considered this subject will feel it to follow as a necessary corollary from the principles above indicated, that the existence of a single real dissimilitude must upset any assumed law, generalization, or theory, previously made upon observation of ever so many similitudes. Thus, had it been announced as a law that all transparent solids exhibit periodical colours, it would have been upset, and therefore proved false, by the discovery of a single case in which a transparent solid did not exhibit such colours. Again, though it had been observed a thousand times, and in a thousand different ways, that water decreases in dimension as heat is withdrawn, and that had been announced as a general law of nature and necessary connexion, yet the very first case in which it was observed that below 40° Fahr. an increase of dimension takes place would have proved the above generalization* to have been an "anticipation" merely.

The reader will feel the exact pertinence of the above observations to the subject immediately before us. The problem is—The Flints of the Upper Chalk: what are the necessary connexions of these in character, mode of formation, &c.? and the point is to explain, upon a general and *universally applicable* principle, the presence, in all its forms, of flint.

To this problem Mr. Bowerbank has offered a solution, namely, "that the common tuberous flints, the horizontal tabular flints, and those forming perpendicular or oblique veins, were ALL produced by the same agency, namely, in all cases, from sponges of which those flints occupy the exact places." I deny that this is a solution of the problem; and, having the principles above glanced at before my eyes, suggest at the beginning of my argument† that, if it can once be shown to be impossible, in any one particular instance, to explain the presence of flint on this theory—if flint is ever, or may be even in a single instance, found elsewhere—the theory ceases to be an explanation of the phænomena, and becomes of no value to the philosophical inquirer; that it is proved to be not an "interpretation," but an "anticipation" merely. To this Mr. Bowerbank replies‡ (instead of re-examining my facts, and ascertaining whether they really are the exceptions I allege) by saying, "This is so richly dogmatical, that one cannot suppress a smile." I have shown that constant regard for such "dogmas" lies at the very foundation of all scientific investigation. Exact science depends wholly on them; natural science becomes more exact by how much such "dogmas" are continually observed. Without attention to them there can be no such thing as *science*; all will necessarily result in mere *empiricism*.

Among the numerous services rendered to science by Professor Owen, none has been more important, as it seems to me, than the distinction he is perpetually enforcing, both in print and in the lecture-room, between *analogy* and *homology*. That distinction, without attention to which comparative anatomy can be no science, but mere empiricism, is founded entirely upon the strong-felt necessity within Professor Owen's mind of continually being guided by such "dogmas" as many of his predecessors have perchance regarded only with a smile. If, on view of an analogy, inquirers would ask themselves whether, in the two or more cases before them, they can for *analogy* read *homology*, they would save vast confusion in science and promote vastly the discovery of truth. Such was my aim in reminding the reader of the "dogma" which has excited Mr. Bowerbank's smile. Numberless illustrations, without the guide of this principle, would have been valueless; with it, any one becomes sufficient.

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* In two articles, "On the Discovery of Truth," and on "The Inductive System of Philosophy," in the 'Christian Review' (Boston, U. S., vols. iv. and v., 1839 and 1840), I have endeavoured to point out the importance and universal application of the principles of investigation above glanced at.

† See this Magazine for January of the present year, pp. 3 and 10. I would also refer the reader to the bottom of p. 9 and top of p. 10.

‡ *Ibid.* for April, p. 251.

Every one knows that sponges are found, among other things, in flints. It is the announcement of this as an explanation universally applicable to the origin and cause of the formation of ALL flints which is the point. I have undertaken to show that that theory is a mere "anticipation," founded only upon a few vague and very loose and casual analogies; and that it affords no true "interpretation" of the phænomena, for that the mass of real facts is full of "dissimilitudes." Mr. Bowerbank has professed to reply to my observations; and the fact that he has done so shows the importance of having the matter thoroughly sifted, in order to get at the truth.

Mr. Bowerbank has, however, not replied to my observations. He does not appear, indeed, to have read my former paper through, as the greater part of my arguments are left wholly untouched, and he has, in very many cases, represented me as saying that which I nowhere say—in many cases the exact reverse of what I do say*. In other and numerous instances, the issues raised and discussed by him are wholly immaterial. I pass by the very distinct intimation, more than once so complacently given, of my hardy ignorance; as that is a mode of meeting an argument sanctioned by such long prescription, that it cannot fail to have its due effect on every candid reader.

In order to save needless length, I shall now show,—1st, in what cases I have been misrepresented or unread; 2nd, in what cases immaterial issues have been raised; 3rd, what arguments have been left untouched; 4th, the unsatisfactory explanations attempted of the facts named and of the specimens figured in my former paper; 5th, some further illustrations which occur to me explanatory of the formation of flint.

1. I certainly did not conceive it necessary to add an interpretation clause to my paper, giving the reader credit for at least understanding that when I used one word, I meant something

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* I beg to refer the reader, for some admirable remarks on this tendency and its mischiefs, to Dr. A. Combe's "Letter to Dr. Forbes," in the 'Medical Review' for Jan. 1847.

akin to such definition as would be found in Johnson's Dictionary; at any rate, not exactly the opposite. Thus, when I named "*peculiar* affinity" (p. 3), I meant "belonging to one to the exclusion of others" [Johnson's Dictionary]. When I used the word "envelope," I meant inwrapping and surrounding, and not mere attachment to, or growing upon, as Mr. Bowerbank understands it (see p. 255).

I nowhere deny the predisposition of sponges for silex; on the contrary, I expressly allude to it several times (see p. 10, &c.). I deny the "peculiar" and "special" affinity only, which the sponge theory requires. The very next clause to that quoted is, "Such facts disprove the alleged *special* affinity." Mr. Bowerbank himself admits (p. 259) that I do not deny what on p. 251 he says that I do deny; thus admitting that the whole discussion raised on the latter page is immaterial and unfounded.

Again, it is said that I do "not offer the slightest explanation of the cause of the suspension in all parts alike of the masses of siliceous spiculæ, the remains of polythalamous shells, small branched corals, and numerous other animal bodies; nor account for the continually recurring presence of that tissue which he (Mr. Bowerbank) has described as, and still believes to be, portions of the skeleton of the sponge, to which the great mass of chalk flints owe their origin." Now any one who has read my former paper will find every one of these questions discussed, both as to the fact of their alleged presence, and as to the probable explanation where present (see pp. 4, 15, 16, &c.). The explanation offered by Mr. Bowerbank himself, in his paper in the 'Geol. Trans.,' p. 183, is clearly untenable, requiring as it does the integrity of the entire sponge tissue, *an integrity which has never yet been found to exist in a single instance.* On the same page (252) I am represented to have described two specimens quite differently from my actual expressions, arising from inattention to the grammatical connexion of my sentences; and am further said to offer no explanation of the fact of the position of certain shells on flint, while on the very page referred to I make an express reference, by note, to an explanation in a later page, which explanation is consistent with all known facts, though it happens to be in direct antagonism with the sponge theory. On the same page I am said to speak of a "nodule of water," an absurdity which is certainly nowhere to be found in my paper.

On page 253 I am represented to consider as "very improbable," and to indulge in certain "fallacies of imagination" respecting, certain habits of sponges, which are so familiar to every one that it never occurred to me to be necessary even to allude to them; much less did I doubt them.

On page 254, by citing the first three lines only of a paragraph, I am made to appear to refer to a wholly different and exactly opposite class of facts to that to which I do refer, and my reference to which (p. 5) is left altogether unnoticed. I show that shells are, in flints, found where they could not possibly be found in sponges. Mr. Bowerbank's observations only further show that, in recent sponges, they are actually found in a wholly different place and manner to what they are found in flints. On the same page, by reversing the use of italics, my meaning is reversed. Mr. Bowerbank having alleged, in support of his theory, that one of the orifices of Echinites is *always* found in one position, I show, from reference to individual facts, the erroneousness of this generalization, and therefore of the special inference to sustain which it was made; nor can a smile at the essential dogmas of inductive science lessen the force of those observed facts.

I nowhere deny or dispute, as *throughout* Mr. Bowerbank's paper I am represented as doing, the well-known fact—known to every merest tyro in natural history—that corals and sponges usually attach themselves to and grow upon rocks, stones, shells, and other hard bodies. Such familiar facts it was unnecessary to mention, either for the purpose of my argument or to prevent my being misunderstood. The question is not, to what sponges attach themselves, of which only all the cases cited by Mr. Bowerbank are instances—but (as I have shown, p. 5), what attach themselves to sponges. The cases instanced are moreover cases of attachment, not of envelopment. In the cases of wood, &c. it is really a question of penetration, not only of envelopment. So that in each and every case Mr. Bowerbank has missed the true point, and been arguing only with a chimæra of his own creation. The important distinction between *enveloping* and *attaching to* is throughout lost sight of.

I nowhere suggest, as represented on page 258, the necessity of thermal heat for the formation of flints, nor make the slightest allusion to it.

From smiling at the dogmas of inductive science my meaning is again misrepresented at page 259; my point in the passage cited being simply that, as I could show "some facts of external forms" inconsistent with the sponge theory, the generalization presented by that theory could not possibly be true, no universal law admitting of exceptions.

On page 260 I am alleged to "distinctly recognise the theory of the gelatinous condition of flint," while the three lines just before quoted from my paper most explicitly affirm the reverse; which view—the impossibility of the *gelatinous nature* of the fluid—is throughout and in many places affirmed and endeavoured to be proved by me. It is quite impossible to be more explicit on the point; and it would be as correct to represent an author writing in support of the animal nature of sponges to be an advocate of their vegetable nature, as thus, and so very little to the point, to misrepresent my language and argument.

On page 260 I am said to suppose the existence of "many minute currents," without any evidence of such supposition from my paper, and in direct opposition to my expressions on page 12, where I speak of the "rare occurrence" of the instances.

The pointing out the erroneous representations given of my language and views of course relieves me from the necessity of noticing at any length the discussions raised by Mr. Bowerbank on the points thus erroneously stated; but the fact of such numerous misrepresentations existing argues little for the soundness of the opposing theory.

2. Nothing is more important in questions of science, as well as of law, than to keep the real question always in view, and not to travel out of the record, and so raise what the lawyers call false issues. By raising always one certain, single and material issue only can the truth be got at. Raising false issues, though very convenient when an argument cannot be answered, serves only to—but at all times it does—obscure the truth and confuse and mislead the reader. The present question is with the flints of the upper chalk*. We have nothing now to do with the siliceous masses and cherty nodules of either mountain limestone or tertiary beds. So, every one being familiar with the fibrous appearance of agates, &c., and with the occurrence of undoubted sponges in the chalk flints, any discussion on either of these is merely leading us astray from the point, viz. a universally applicable generalization as to the formation of the flints of the upper chalk.

It may be very interesting, in a natural history of recent sponges, to inform us of all the modes in which they are found. But, unless it can be shown that they are found in flint as they are alleged on page 253 &c. to be found recent, it is wholly beside the mark, and it is unnecessary to travel to Algoa Bay, Torres Straits or Wollongong in search of illustrations. Now it is well known to all familiar with flints, that the latter are never found, and have never been found, having the characteristics here described by Mr. Bowerbank as attaching to recent sponges. It was said of the old Greek philosophers, that, while one made laws for men as they are, another made laws for men as they *ought to be*. So Mr. Bowerbank, having by "anticipation" determined that all chalk flints have been sponges, proceeds to tell us how *recent* sponges are found. Hence we may indeed infer his opinion, that flints ought to be found the same; but, in the mean time, the facts as to the actual mode in which flints are found are altogether lost sight of. With the "habits of the Spongiadæ," such as described on page 253, and figured by Dr. Johnston and others, every one is familiar; but they have nothing to do with the question before us, unless indeed it be to afford another illustration of the utter fallacy of the sponge theory, by satisfying us that there is no true analogy whatever between the flints and any of the illustrations given or cited. Of that fact every one having a moderate acquaintance with flints must be fully aware.

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* In all geological questions, nothing is more important than stratigraphical exactness. A recent instance of neglect of this has occurred which will be a fertile source of error and vain theory. In the second volume of the 'Journal of the Geological Society' is a description of a Pterodactyle, alleged to be from the "upper chalk." Immediately after it appeared (Feb. 1846) I informed the author that this was incorrect, as *no upper chalk whatever* exists in the localities whence his specimens came. No correction has however been made, and it has now gone forth to the whole scientific world, with the authority and sanction of the Geological Society of London (p. viii. of vol. ii. of the Journal), that Pterodactyle remains have been found in the upper chalk, a statement fruitful of future theories, and which is without a shadow of foundation.

The facts cited on page 254 again, being only adduced to appear to meet a sentence which, in its original connexion, had an exactly opposite meaning to that here given to it, are equally beside the mark, as they are also equally inconsistent with any of the observed phænomena of flints.

I have already shown that the whole discussion on the gelatinous state of the fluid is gratuitous; but I must remark that Mr. Bowerbank involves himself in several contradictions on this point, first ridiculing the idea of one solution preserving any integrity within another* (page 260), and afterwards speaking (page 262) of the "great prevalence in solution in water" of this very substance. Mr. Bowerbank cannot surely but have seen the condition when mixed of fluids of different densities, as oil and water, &c. &c. The specific gravity of the liquid silex is obviously a very material point. But Mr. Bowerbank himself, in his paper in the 'Geol. Trans.,' p. 183, several times mentions the "gelatinous globules," so that his views must have undergone a material change in this respect.

3. But all the material arguments used by me are left wholly unnoticed by Mr. Bowerbank. This silence can leave but one inference. It is proper, that the nature of the discussion may be understood, to enumerate the principal arguments thus unan-

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* Dr. Turner expressly alludes to the "tendency of like molecules to get together and adhere while intermixed with a mass of dissimilar matter rendered liquid," and the fact is familiar. Mr. Bowerbank's observations amount, if anything, to a denial of molecular attraction!

swered. On page 3 of my paper is discussed the necessarily implied *peculiar* and *special* affinity of sponges for liquid silex; on the same and following pages, the sparing and fragmentary presence and character of the remains of tissue; on the latter page, the non-existence of any places of roots, and the contrary fact of undoubted sponges being found in which such places are always present. On page 5, not only is the abundance and size of shells, &c. found *within* flints noticed, but the position of the organic bodies found on the exterior of flints is very fully and prominently discussed. On page 6 the condition of certain zoophytes is noticed. On page 7, the important fact that—and it is explained why—shells fixed on Ventriculites are left bare by the flint, and the fact that soft animal matter, and not, as in recent sponges, shells and other hard substances, formed the point of attraction for the flint. On page 8 the phænomena of Ventriculites are more fully noticed. On page 9, the important fact of flint occurring to the same extent without as within the Ventriculite, though with no connexion between them; and, on the same page, the extremely frequent case of flint nodules being found round the roots of Ventriculites, where sponges could never reach, but which places would be peculiarly apt, according to my view of the formation of the flints, to exhibit these aggregations.

The non-sponge character of Ventriculites is also shown on page 9*, and also certain inconceivable phænomena which the sponge theory requires (among many others) to explain the silicification of Ventriculites.

The true nature and character of the specimens figured is described on page 10, &c., and the mode of their formation endeavoured to be explained in detail; and it is proved that the liquid silex could not possibly have been in a gelatinous state, but must, on the contrary, have been in a state of extreme liquidity, and liable to very rapid solidification; which rapidity of solidification is shown to be necessarily inconsistent with the sponge theory, and which Mr. Bowerbank's remark (page 250), on the very enduring character of the horny skeleton of sponges, makes still more evident.

On pages 15 and 16, the modes and forms assumed by the flint nodules† and tabular masses, and the imbedment in them of the various classes of organic remains, are explained, the presence of the fragmentary tissue having been before explained on page 4.

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* Mr. Bowerbank's alleged recent Ventriculites (p. 258) have no analogy whatever to the true Ventriculites; nor are his specimens so new or rare as he imagines, the most striking of them being beautifully figured, with others no less striking, on plate 59 of Ellis's Zoophytes.

† See note *post*, p. 308.

Not one of the arguments and points thus enumerated, and minor ones might have been added, is attempted to be answered by Mr. Bowerbank. That fact naturally gives them much additional weight, as his paper is an avowed "reply" to mine, and all that could be answered would assuredly be so:—*Expressio unius est exclusio alterius.*

4. Let us now see how points really touched on, and bearing on the question, are handled.

On page 250 there is an attempt to reconcile the facts of the unequal distribution of the sponge spicula [their "suspension in all parts alike" (see page 252*) is a thing which does not exist]. When I speak of the "destruction of the structure of the sponge," I of course allude to the "tissue" so often named; though, whether this or the gelatinous matter be meant, it is equally certain that its destruction is a necessary postulate of the sponge theory, inasmuch as, if not destroyed, it would be still present; but no single instance exists in which that tissue has been found to pervade the whole substance of any flint; and this must be obvious to every one familiar with the phænomena, notwithstanding that Mr. Bowerbank now "denies totally†" this destruction of structure. It is admitted, in this very passage, that the "horny skeletons are very enduring," and see further page 258. This fact then necessarily implies, what I have shown to be inconsistent with fact, and what the author himself elsewhere now denies, viz. the long-continued gelatinous state of the flint to allow of the decomposition of this tissue to such an extent that only the fragmentary remains we have of it shall be left. Mr. Bowerbank tells us that the spicula exist in the gelatinous substance. Dr. Johnston, on the contrary, describes those spicula as "bound together by a substance analogous to horn or albumen," either in the form of fibre or diffused, this horny matter being the very substance described by Mr. Bowerbank as so "very enduring;" so that the spicula ought clearly to be retained in their position, and not to run away in the poetic manner imagined by Mr. Bowerbank. Besides this, Mr. Bowerbank tells us‡ that the mealy coating of flints is formed by the penetration of the surface of the sponge by fine silt, which caused the preservation of form. If such a coating were thus formed, it is obvious that the spicula could not escape in the way represented; and the fact is, further, that spicula are not found specially to abound in the mealy coating, as the explanation offered would, at any rate, render necessary.

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* See also 'Geol. Trans.,' vi. p. 181. This very assumption is also obviously inconsistent with the explanation now offered at p. 250.

† Ehrenberg expressly says of the Paramoudras, that he "*failed* to discover in the interior the structure of well-preserved sponges;" and further, that "the internal structure does not in any way favour this view" of their spongeous origin. This is not an unimportant testimony. See 'Ann. Nat. Hist.,' vol. ii. pp. 161 and 162. In the 'Geol. Trans.,' vol. vi. p. 183, Mr. Bowerbank himself says, that often "no indication of the former presence of the organized structure of the sponge remains." What are we to understand by such contrary and contradictory propositions?

‡ Trans. Geol. Soc. vol. vi. p. 184.

I shall presently show, moreover, that Mr. Bowerbank is entirely wrong in imagining that the gelatinous substance was poured out of the sponges before they were enveloped in flint.

On pages 252 and 253 Mr. Bowerbank tells us that the position of such shells as I described on pages 4 and 5 is "quite natural" on the outside of sponges. I answer that it is contrary to every law of gravity and to every fact, and that no illustration of similar facts can be produced among recent sponges. He himself says, just before, that their natural mode of sinking would be to the bottom of the water*. So I say, and beg to refer to the argument on page 5 of my former paper, which has not even been noticed by Mr. Bowerbank. He has shown to me, I must presume, having visited his museum twice for the purpose, all his most characteristic specimens; and I have certainly not seen in his collection, or elsewhere, a single specimen having the slightest approach in character to the specimens described by me on pages 4 and 5. It is indeed obviously impossible that shells could adhere to any sponge in such positions as in the second-named specimen. The fact of those shells being so found is, on the other hand, a beautiful illustration of the mode of formation of flint suggested by me, namely by affinity for a nucleus combined with molecular attraction. On Mr. Bowerbank's theory, these shells ought, of all places, to be covered with the sponge, sponges especially attaching themselves to solid bodies. But the fact is, that instead of the flint "loosely embracing" (page 253) a shell lying on it, as in the cited sponge, the presence of a shell on the surface of a Ventriculite invariably acts as a *repellent* of the flint. The flint surrounds the shell on all sides, tends to lap a little over in a bulging manner, but does not extend over. Why is this? Because the soft body had a peculiar affinity for the liquid silex, the shell had none. Attracted by the soft body, the flint surrounds it—extends as far as the shell; the molecular attraction causes it to bulge a little over the margin of the shell; but the combined nuclear affinity being wanting beyond, it extends but little way. This seems very simple; and as it is the test of a true theory or generalization, that every new fact, thoroughly understood, serves only to illustrate it and make it still clearer; while it is that of a false theory or generalization, that new facts cannot, without much distortion, be reconciled; so these phænomena, while negativing in the most absolute manner the sponge theory, beautifully illustrate the mode of formation of flint which I have ventured to suggest.

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* We are not speaking here of such shells as Ostreas and Dianchoras, which affixed themselves by special provisions to surfaces of Ventriculites, &c.

The reader must again carefully note that we are not, in truth, considering the cases of sponges *growing onto, attaching themselves to, shells*, &c.—such cases as are figured and described in Dr. Johnston's beautiful and most interesting volume. It is notorious, and is not attempted to be denied, that we never do find flints in which a root-place is visible, except in such instances as I have noted on pages 4 and 9. We are considering shells scattered on all parts of the exterior of the flints*.

While on this point, it is proper to notice that Mr. Bowerbank bases his ideas of the sponge growth of the tabular flints (Geol. Trans. vi. 184) on an entirely erroneous view of the nature of these tables. They are, in fact, all made up of *two distinct plates*, as I shall have occasion to notice hereafter†. This alone renders their sponge origin quite untenable.

It is laid down by Mr. Bowerbank on page 255, as it is by Dr. Johnston (page 11), and is no doubt correct, that if one sponge grow over another of different species, they *never unite organically*; and hence he would explain the Wiltshire flints. The empty space there however is too great by far for such an explanation, as the parasite clings close though it does not organically unite. But this explanation is wholly inconsistent with another very large class of facts. For it is more important to observe, that, while Mr. Bowerbank is obliged in another place (page 259) expressly to admit that one species of sponge was so extremely different from another, that, though the inclosed species is often wholly preserved, the incloser is utterly decomposed (notwithstanding the "very enduring" nature of its horny skeleton), the fact is—I speak from inspection of many hundred specimens in my possession—that hardly a single instance, if any one, can be found in which these so different species of alleged sponges have *not* united organically‡; insomuch that it is only by artificial means that the external structure, either outside or inside, of the inclosed object can be detected. One of the two generalizations thus, with equal confidence, announced, must therefore necessarily be false; and none who see the structure of the inclosed object perfectly preserved, while not a trace of the structure of the alleged incloser can be found, can for a moment doubt which generalization must be abandoned.

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* I do not here dwell on shells inclosed in flints, as they present no difficulty.

† See *post*, p. 308. Ehrenberg, though not noticing this point, felt their sponge origin untenable. See 'Ann. Nat. Hist.,' vol. ii. p. 162.

‡ In a few instances small patches may be found in which they can be separated, but these are extremely rare. The cause of such instances is quite explicable on principles hereafter to be explained, which also explain the Wiltshire specimens. See p. 306.

But we come now to another and most important point, to which I request the special attention of the reader; and I cannot imagine that any one who applies the rules of inductive philosophy to scientific investigation can contemplate the facts I have now to name without at once feeling that the sponge theory is utterly untenable. It is necessarily a postulate of the sponge theory, and is distinctly stated by Mr. Bowerbank (page 255, &c.), that, in every case where a Ventriculite is encased in flint, it is in consequence of the *parasitic growth over the Ventriculite of a foreign sponge.* I have in my cabinet at least 500 specimens (probably very many more) of Ventriculites. I have shown Mr. Bowerbank the greater part of these, and he was not able to point to more than one single specimen (and that very doubtfully*) in which a parasite had not overgrown the original body, and so formed the flint. Now it is not enough to tell us of instances of parasitic sponges, nor yet that certain species are always parasitic. It must be demonstrated that there existed species, having a special and peculiar affinity for silex, which were invariably parasitic; and that there was invariably present silex in precisely sufficient quantity to saturate them. It must further be proved that this species of parasites had the wonderful property of always growing in pairs, one growing inside and the other outside of the invested object, and yet having no possible connexion with each other, though, with very rare exceptions, growing to precisely the same level. It must further be proved that the Ventriculites themselves, unless thus invested, never could become silicified; that, when invested by the parasite, they were always silicified. It must yet further be proved that the parasite, though retaining its form, invariably decomposed utterly before that process commenced in the invested Ventriculite. Further, it has been seen that the law of non-organic union of different species is never observed by these particular species of assumed parasites. This is a pretty long draft on our credulity. Now I request the reader to note that, having before me hundreds of Ventriculites invested with flint, and hundreds not invested with flint, there is not one solitary instance in which any parasitic sponge is found *in the chalk* and unsilicified to surround a Ventriculite; nor is there one single instance in which, the Ventriculite being invested by flint, a trace of the alleged parasite is found beyond the margin of the flint, as is the case in numberless instances in true sponges. But it is continually found that as well Ventriculites as sponges of many different varieties are preserved in the chalk equally as in the flint, and that without any connexion whatever with Ventriculites, but always themselves again invested with the assumed parasites. It is further to be noted, that there is not a single case in which a Ventriculite or sponge is found silicified in a normal form, i. e. *without* an assumed parasite; nor, further, a single case in which either Ventriculite or sponge is found inside the assumed parasite in the unsilicified state in which each is so often found in the chalk, and with the body or extended threads of the assumed investing parasite alone silicified. What an extraordinary tissue of contradictions is then involved in this theory! Had such parasites existed, no one can doubt that they would have been often found investing Ventriculites and sponges in the chalk and unsilicified; and, further, that Ventriculites and sponges would have been found silicified indifferently with or without parasites, as well as unsilicified within the silicified parasite. The doctrine of chances, which is but another name for the laws of unity and design and the principles of induction, is at an end, if, to prop up a theory, we are called upon to accede to postulates thus monstrous and contradictory, but absolutely necessary if the sponge theory is to be sustained.

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* This case really forms no exception. Mr. Bowerbank declares even the specimens named by me at bottom of p. 8 and top of p. 9 to be invested with threads of the parasitic sponge.

I have alluded in my former paper to the frequent flint nodules formed round the roots of Ventriculites. To my no small surprise Mr. Bowerbank showed me a specimen of this kind, removed at the lower part from the chalk; and pointed to the orifices through which the roots passed as proof that it had been disrupted, and as great excurrent canals. I expressed my astonishment as strongly as courtesy would permit. I stated that I would engage that the assumed excurrent canals would, if cut open, show roots of Ventriculites. Mr. Bowerbank, however, insisted on the excurrent canals. Now, having hundreds of like specimens in every condition, the true facts are so familiar to me, that I can only be amazed at this as an extreme instance of the lengths to which "anticipation" will lead a man to overlook the most obvious facts in support of his theory. Mr. Bowerbank, moreover, insisted to me that, where flints were formed round roots of Ventriculites, the latter must have been disrupted and lying on their sides; remarking that it must be so, *because* otherwise the sponge could not have grown round the roots, which, normally, struck into the mud. This is indeed arguing in a circle. I admit the latter part of the position, namely that no parasitic sponge could have grown into the mud and so invested the roots; and, because it is so, I again assert that these flint nodules never were sponges. For I am prepared to demonstrate to any one who will favour my cabinet with an inspection, that the Ventriculites thus invested as to their roots were *not disrupted,* but, on the contrary, were growing normally and *in situ* when invested by the flint: and the great specific gravity of the flint readily accounts for its sinking into the soft mud so as thus to invest their roots. It is further obvious, that had they been, as alleged, disrupted and lying on their sides, the accumulations of flint would be all on one side, they forming merely the attachments of the sponge; whereas the fact is, that the general rule is a pretty equal distribution of flint all round. If a special mass lies on one side, it is capable of explanation.

My space warns me that I must hasten to notice the explanations attempted of the figured specimens.

Mr. Bowerbank would have us believe (page 257) that the woodcut (page 11) is explained by the flint being "the remains of one of the large internal canals of the sponge." When the reader is reminded that this is an assumption of a *petrified mass of revolving water* (revolving, too, normally, after all the rest of the sponge has utterly decayed), it cannot be necessary to notice the suggestion further.

I noticed Mr. Charlesworth's silicified pulp-cavity with a reservation. I have seen the specimen. There are undoubted polythalamia in it, though not such apparent sponge tissue. For the reasons named in my former paper, I see no difficulty in accounting for the presence of the minute polythalamia, though very great in imagining the growth of any sponge in such a locality. As to my own figured specimen of jaw, I certainly was not then, nor am I now, aware that the substance of the teeth and jaw is silicified, but know the reverse to be the fact; and when the reader learns that the teeth in that specimen are anchylosed to the jaw, which is solid below them, he will feel that no "space intervening between the tooth and its socket" ever existed. But Mr. Bowerbank's attempts to explain figures 1, 2 and 3 are the most extraordinary. The condition of figure 2 is so fully explained in my former paper as at once to refute the notion of an "accident during the elevation of the chalk,"—a notion under any circumstances so far-fetched and, on every ground, inadmissible, that it cannot be necessary to combat it. Mr. Bowerbank cannot, however, have read the description of this specimen. As to fig. 3, of which it is most gratuitously said (page 259) that some of the fragments "appear to have been shells," in direct opposition to the fact, Mr. Bowerbank is compelled to admit that the fragments are "fragments of older flints," which is just what the specimen was engraved to show, and which Mr. Bowerbank leaves wholly unexplained. As to figure 1, I cannot at all understand the statement that it "exhibits an appearance of having been deposited in concentric layers, which are exposed by what seems to have been an irregular decomposition of its surface." I beg, in the most explicit manner, to say, that not the slightest appearance of the kind, in any respect whatever, is presented. Let the reader glance over the description given on page 11 and examine the plate, and he will at once see that the actual appearances are the most directly opposite and reverse of the alleged appearances. The decomposing flints alluded to are not very rare; but in every respect whatever they totally differ, both in external aspect and internal appearance and arrangement, from the specimen described, of which all the edges are perfectly sharp, the substance perfectly hard and in the highest state of integrity, and the *horizontal*—and no trace of concentric—layers very visible in some places. I have since obtained other fine specimens of the same kind, all in an equal state of integrity, and all therefore equally conclusive against the sponge theory.

Mr. Bowerbank is much distressed at the idea of "currents and whirlpools." If I gently give motion to a jug of water at a given temperature, the whole mass assumes a form, as stated in my former paper, very much like that on these specimens, and the necessary "currents" excite no marvel. The motion among the particles themselves causes the angular ridges. It is only when the mass assumes special forms, as in fig. 1, that some special motion must be suggested. Now I can show that such slight, but sufficient, motions took place in the region of these phænomena, having been at the pains to collect specimens for the purpose of establishing the point. Thus all the difficulty vanishes. There are no "contrary currents in opposition to the figured side," but precisely the reverse, as stated in my former description of the specimen.

As to the last-named specimen (fig. 1), it may not be amiss to state that, as if to afford direct evidence of the truth of the views advanced by me in explanation of the phænomena exhibited by that and similar specimens, I have been fortunate enough, since that paper was written, to obtain a specimen exactly similar to the part *a* in fig. 1, *in the chalk*. This proves the existence of an absolute cause in that ocean sufficient for the production of such forms. Now it is particularly interesting to find that, in this chalk specimen, immediately underneath the chalk surface, which was cleared from the block by my own hand, there is a layer of flint—a capsule fitting into but below it, so that the chalk impression is no mould, but a cap. This is beautifully consistent with the suggested cause of the solidification of flint under such circumstances, viz. the given motion. It further suggests how the chalk impression remained firm after the motion which caused it had ceased, the rapidly solidified flint compressing it.

5. Space will not allow me to add many of the additional illustrations which crowd upon me. Several fresh facts have been already named. I will therefore conclude with a brief notice of two most important facts in the natural history of flints and their inclosed bodies, which, by an extended series of preparations, I have ascertained since my former paper was written. Each of these points has furnished me with direct evidence of that which I had before inferred, as most probable, from collateral facts only. Each equally demonstrates the fallacy of the sponge theory, and points to important truths in the natural history of flints. As I propose to take an early opportunity of detailing the observations relative to the first point alluded to, I will only call attention here to the general result.

Flints are generally considered in the light of homogeneous masses, mineralogically the same throughout each individual mass, except in such cases as Mr. Bowerbank notices on p. 257, and which notice by him shows that he is not aware of the facts which I have now to state*. Indeed had he been so, the sponge theory could never have been framed. Mr. Bowerbank speaks (p. 257) of chalcedonic crystallization "wherever there has been a small space originally *not* occupied by spongeous substance;" and at p. 250 he evidently implies that silicification took place after all animal matter had drained out of the horny skeleton; and at p. 258 he cannot understand any reason for imagining the Ventriculites to have been imbedded in flint while alive. His remarks also, in this and other papers, upon agates, equally show his opinion that the chalcedonic crystallization took place *only* in spaces "originally *not* occupied by spongeous substance." Now I am prepared to show that, confining ourselves to the *flints* of the upper chalk, our true point, the exact converse of this is true, and that the places in which chalcedonic crystallization has taken place are places which *were originally occupied by the spongeous or other* animal substance†. I will not at present absolutely affirm that in no other cases is chalcedonic crystallization found, though I believe such to be the case. It is enough for my present purpose if I can clearly show that this chalcedonic crystallization is present in numberless instances in perfectly solid flints, and that, in all such cases, I can demonstrate the origin of the space it occupies. In my former paper several facts were named (pp. 4, 7, 8, 9, 16, &c.) showing that the places of the soft parts of animals had been penetrated by silex. I felt however and admitted a difficulty in fully explaining this. That felt difficulty led me to make a long series of preparations, which seem to have at length cleared up the point and thrown much light on the whole formation of flint.

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* Nor are these phænomena noticed in Brongniart's 'Essai sur les orbicules siliceux;'—their possibility being indeed expressly put out of the category. See pp. 21 and 23 of that 'Essai.'

† It will be understood that I mean to express this as *the ordinary cause* of hollows in chalk flints, which Mr. Bowerbank nowhere explains. Elsewhere (in volcanic rocks, &c.), hollows existing from other known causes, siliceous infiltration and crystallization would obey the ordinary laws.

If a section of moderate thickness is properly taken from a flint in which experience teaches the observer that a Ventriculite or true sponge has been inclosed, it will be found, in holding the specimen up between the eye and the light, that a part of it, having a very defined outline, is much more transparent than the remainder. On examining that more transparent part under an inch achromatic, traces, more or less distinct, of crystallization will be obvious. Continued and extended observation—the means of which I shall be most happy to supply to any who may feel interested in the question—is necessary to realize the result, which will however in the end inevitably be the conviction that those lighter shades are the places formerly occupied by the soft parts of animals or sponges once inclosed—in a living or undecomposed state—in the flint which had suddenly and solidly encased them in an amorphous condition; that the soft parts first, and afterwards the firmer parts, of those animals subsequently decomposed, *leaving*, in the *then solid flint which encased them, a hollow mould of the exact form of the living animal*; that subsequently to this it was that silex, suspended in a gaseous or liquid form (I incline to think the former the more frequent*), found its way into these then hollow spaces and there crystallized†; that, where any of the firmer fibrous parts happened to remain, they served as nuclei round which crystallization formed; that if the silex were thus presented in sufficient abundance, it gradually filled up the entire hollow space, leaving a perfectly solid flint; if not so abundant it crystallized round the remaining fibres only, presenting us with those open reticulations of such exquisite beauty with which carefully-made sections have most amply rewarded my labours in the course of these preparations.

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* Mr. Bowerbank's objection (p. 258) to thermal heat is unfounded. Shells, &c. may be subjected to a greater heat than that necessary to suspend the silex in a gaseous state without danger. Dr. Mantell has reprinted some interesting experiments on the suspension of silex in steam, and, though this does not seem *necessary*, the permeation of rocks by heated gases is so well known, that it presents perhaps a more obvious general (not universal) explanation of the gradual accumulation of the matter than any other hypothesis.

† The note (p. 6) applies in all these cases. If there were no orifice, neither could gases of decomposition escape nor the chalcedony enter. The place of the original orifice may always be found, either marginal or radical.

In Dr. Turner's lecture (p. 27) he says, "The development of regular crystals was owing to the extremely slow progress of the same process which, when less slow, might cause the deposits to be amorphous." This precisely accords with the view suggested of the rapid solidification of the amorphous flint round the living animal, and the slow deposit of the crystallized silex * in the subsequently present hollow space.

Of course cases occur in which other substances besides chalcedony have insinuated themselves into the hollow. Such cases—the line of division being equally marked—serve only further to illustrate my position. In the majority of cases however it is chalcedony.

To these observations on this first point I will add nothing more at present but the remark, that the suggestion offered in my former paper (p. 4) as explanatory of the presence of *fragments* of sponge tissues in many flints has a strong light thrown on it by these observations, and the bearing of these observations on the present question becomes of great importance when those fragments are considered. I there considered them to be torn fragments of the horny tissue. In such case it is clear that none of the gelatinous substance of the sponge would be present. It would be purely and truly a mere fragment of the horny network. Consequently this would be indeed saturated with and imbedded in the flint itself, and, there being no gelatinous mass to decompose, no hollow space would ever be left to be filled with chalcedony. *Hence it is* that around *such tissues* we see none of the semitransparent chalcedony, but simply the dull homogeneous flint. Hence the true nature and origin of those fragments, as suggested in p. 4, is—with the consistency which always attends the examination of new facts by the light of a true explanation of nature—rendered almost demonstrative, and, by consequence, the so-called "characteristic flint tissue" at once placed out of further question and removed altogether from the argument. And yet it is upon the presence of these fragments only that the whole fabric of the sponge theory has been raised! Even the weak analogy which they afforded, and on which the whole "anticipation" rested, seems thus entirely destroyed; and the facts which destroy it appear to increase the light which points with clearness to the true origin of the modes and forms in which the flints of the upper chalk are actually found, as suggested in my former paper, namely, by the rapid solidification of a fluid—induced by special circumstances—combined with its own molecular attraction; those special circumstances being, generally, an affinity for an organic nucleus*; occasionally, mere mechanical action, such as motion taking place where the solution happened to be present. The point before obvious as a fact, but difficult of explanation, of the penetration of soft parts, has now also, I trust, received some elucidation.

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* For convenience this more transparent part may be termed chalcedony, though it is clear that chalcedony and flint are merely different forms of silex, having a differing amount of foreign matter and impurities. The gaseous state of the chalcedony would tend to cause it to have less of the grosser impurities which discolour the flint and render it opake. Hence its general transparency.

The second point to which I would call attention relates, not to the formation of the nodular flints, in which the bodies just considered are usually found, but to a no less interesting and confirmatory fact as to the suggested origin of the tabular flints. I suggested, at p. 16 of my former paper, that "where there was a layer of minute organic bodies, there would be fewer centres of attraction, and tabular flints would be formed." This has been confirmed by facts of subsequent detailed observation. The tabular flints, which extend unbroken over vast spaces, are not found as described by Mr. Bowerbank, 'Geol. Trans.' vol. vi. p. 184, but are formed of two plates united in few places. Between the plates, and totally unconnected with the chalk above or below, is a layer of pulverulent substance, *not chalk*, but which, on submission to the microscope, is found to be made up of most minute organisms—so minute as only to be at all discernible under a quarter-inch achromatic, and only clear under an eighth. These are of a character and belong to genera totally distinct and different from the ordinary microscopic forms abounding in the beds of chalk immediately above and below the several tables, while the latter forms are extremely sparing in this most interesting and remarkable layer. The principal forms in this are extremely numerous and minute Gallionellæ, a minute Navicula, and several other forms. These forms make up the whole mass, or very nearly so. These facts point to phænomena, as to the time and manner of the deposit and depth of the water, which require careful and separate consideration, and on which therefore I forbear now to enter. Their important bearing on our immediate subject cannot fail to be recognised. Not a trace is found on the under side of these tabular flints of the nature described by Mr. Bowerbank (p. 254) as appertaining to flat sponges, while the remarks made by me on p. 5 are most fully borne out. I have had an opportunity of examining a large area just cleared of the flat table of flint. It was perfectly flat, as was the upper surface also of the flint removed. No shell of any large size was found on either surface. I observed however in several places on the upper surface lines of much the same character—though occurring for the most part only singly or few—as the lines on the specimen figure 1, and which lines I cannot doubt owe their existence to a slight motion of the water above while the mass was in the act of solidification.

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* As to the formation of the nodular flints by "separate centres in one mass" (p. 16 of my former paper), this will surely be understood by any one who has watched two or more globules of water or mercury approach and unite. Had they contained organic nuclei, instead of forming fresh and larger perfect globules they would have formed irregular nodules like the flints.

In conclusion I would remark, that, while it has been absolutely necessary for me, in order that some approach might be made to the discovery of truth on this very interesting and oft-mooted question of the formation of the flints of the upper chalk, to show how much my language and views have been misrepresented, and how contrary the facts are to the representations, I would not for a moment be thought to imply any intentional misrepresentation in either respect on the part of Mr. Bowerbank. It is one of the greatest evils of all "anticipation" that it tends to dim the sight and to warp the impression derived from what seems the clearest fact or statement, and that any fancied analogy (I use the word as more familiar than *similitude*) is seized and dwelt on to the exclusion of numberless points of dissimilitude. I trust that the observations of Mr. Bowerbank may serve to advance the discovery of truth in this matter, if they be the occasion of my rendering more clear the facts and reasons which appear to me to demonstrate the existence of numberless dissimilitudes between the facts of nature and the generalization hazarded by him, and of enabling me to render also clearer the suggestions which I have ventured to throw out, and which I would fain hope tend to elucidate in some degree the natural history of the formation of the flints of the upper chalk.

XXXIII.—*Descriptions and Notices of British Shells.*

By J. Gwyn Jeffreys, F.R. & L.S.

It was my first intention to have reserved for the work on British Mollusca, in which my friend Mr. Forbes requested my assistance, the publication of any discovery on this subject which may from time to time have occurred to me; but as in his opinion the anticipation by this mode of publication of any such discoveries would increase, instead of diminishing, the interest of the proposed work, I will give a short account of some important additions to the Catalogue of British Shells, for which I am principally indebted to my indefatigable friend and collaborateur, George Barlee, Esq., during his excursion to the western coast of Scotland last year.